Chapter 4

Macrocognition, Mental Models, and Cognitive Task Analysis Methodology

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Introduction

In one view of macrocognition (Klein et al., 2003), the "primary functions" are things that domain practitioners say they need to accomplish: replanning, sensemaking, decision making, and so on. The "supporting processes" are cognitive capacities that make possible the achievement of the primaries. Mental modeling is such a supporting process, especially critical for sensemaking (Klein et al., 2006). This chapter is an exploration of some methods for gathering data, representing, and studying mental models. (In this chapter, we do not review methods of knowledge elicitation, broadly conceived. Reviews have been provided by Cooke (1992, 1994), Crandall, Klein, and Hoffman (2006), Hoffman and Lintern (2006), Hoffman et al. (1995), and Olson and Reuter (1987).)

Our purpose in this chapter is fairly immediate and practical: To discuss methods and methods for studying mental models. We discuss tried-and-true methods, but we also present some ideas about new methods, with an invitation for researchers to apply and evaluate them. This is a practitioner's account, that is, a description of a battery of techniques that researchers have developed for dealing with the problems associated with eliciting and refining meaningful and usable accounts of expert cognition. Our stance is "practitioner confessional" rather than a dogmatic "this is how it should be." This chapter is basically an invitation to naturalistic empirical inquiry: If one were conducting cognitive task analysis (CTA) on experienced domain practitioners, for purposes such as cognitive systems engineering, what sorts of methods might be used to reveal mental models?

First, however, we do need to deal with some philosophical issues. Any treatment of mental models could go into great length about issues of scientific meaning, validity, relation to phenomenology, and so on, and could rehash a considerable literature on the scientific status of mental models. With this in mind, we take pains to be succinct.

A History of Mental Models

The mental model notion, or something very similar, can be found in the works of Thomas Aquinas' *Summa Theologica* (1267), where he argued that, phenomenologically speaking, concepts are "in-formed" in the mind and

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"re-presented" to consciousness. In his application of the mental models notion to the field of business organization and management, Jay W. Forrester (1971) made Aquinas' point using modern language:

> A mental image or verbal description in English can form a model of corporate organization and its processes. The manager deals continuously with these mental and verbal models of the corporation. They are not the real corporation. They are not necessarily correct. They are models to substitute in our thinking for the real system that is represented. [Forrester, 1961, p. 40]

The mental image of the world that we carry in our heads is a model. We do not have a city or government, or a country in our heads. We have only selected concepts and relationships, which we use to represent the real system (Forrester, 1971, p. 213).

It has been suggested that Ludwig Wittgenstein was proposing a mental model notion in his discussion of the language, in which he contrasted a "picture" theory with rule-based theories (Mental Models website, 2007, p. 1). E.C. Tolman's (1948) notion of a "cognitive map" is often cited in the mental models literature. Kenneth Craik, a pioneer of the information processing viewpoint, talked in his book *The Nature of Explanation* (1943) about explanatory models as having a "relational structure" similar to the thing being modeled (p. 51):

> The idea that people rely on mental models can be traced back to Kenneth Craik's suggestion in 1943 that the mind constructs "small-scale models" of reality that it uses to anticipate events. Mental models can be constructed from perception, imagination, or the comprehension of discourse. They underlie visual images, but they can also be abstract, representing situations that cannot be visualised. Each mental model represents a possibility. Mental models are akin to architects' models or to physicists' diagrams in that their structure is analogous to the structure of the situation that they represent, unlike, say, the structure of logical forms used in formal rule theories. [Mental Models website, 2007, p. 1]

The mental model notion can be seen in the emergence of American cognitive psychology, in the debates about the "psychological reality" of theories of mental representation in the 1970s and 1980s (for example, Pylyshyn, 1981). The two seminal books on mental models (Johnson-Laird, 1983; Gentner and Stevens, 1980) used the notion to explain high-level cognitive capacities, especially expert–novice differences in knowledge content and organization. Since then, the mental model notion has seemed useful in many studies of cognition and learning, including research on sentence comprehension, research on deductive reasoning and argumentation, and research on causal reasoning (for example, Geminiani, Carassa, and Bara, 1996; Green, 1996; H.A. Klein and Lippa, in press, 2008; Legrenzi and Girotto, 1996). Mental models (of various stripes) have been regarded as fundamental to the field of cognitive engineering, in such topic areas as "user models" in software and interface design (for example, Ehrlich, 1996; Norman, 1986; van der Veer and Melguzio, 2003) and mental models as the basis for knowledge bases and rule systems in the development of expert systems (Scott, Clayton, and Gibson, 1991). The mental model notion has spilled over to other communities of practice in addition to the knowledge acquisition community, such as business systems dynamics research:

"Systems dynamics researchers have devoted a substantial portion of their research effort to developing a wide variety of techniques and procedures for eliciting, representing and mapping mental models to aid model building…[Mental models] are the 'products' that modelers take from students and clients" (Doyle and Ford, 1998, pp. 3–4).

For a full view on the nature of mental models one can also look to the large literature in philosophy of science on the relation of theories, metaphors, and models, and various kinds of models (mechanical, analog, conceptual, isomorphic, and so on) as mediators in scientific understanding (for example, Black, 1962; Hoffman, 1980).

In addition, within a number of disciplines one can find independent "discovery" of the mental models notion, as for instance in weather forecasting, where the notion of the forecaster's "conceptual model" has been used for some decades to distinguish understanding from the computational weather forecasting models (see Hoffman, Trafton, and Roebber, in press).

What Mental Models Are, Are Not, Might Be, and Might Not Be

There are several reasons why the mental model notion is controversial.

Mental Models are Mental

The mental model notion 'wears its mentalism on its sleeve,' making it a target for criticism and debate, especially in the United States where we still experience a lingering hangover from behaviorism. The argument is that mental models cannot be observed directly in "pure" behavior, and so like all phenomena of mind, they are of dubious scientific status (Rouse and Morris, 1986). For Europeans, less affected by the scourge of behaviorism, the mental model concept emerged gracefully. Our view is that the "mental model" designation/metaphor is invoked out of recognition of a phenomenon, not to be brushed aside for being subjective, or to be avoided because research on mental models fails to qualify as good science. To assert that the mental models notion has no explanatory value is, to us, to merely choose to ignore the obvious. Those who do that are walking a different road than the one we walk. Our challenge is to empirically explore, understand, and explain things that phenomenology dishes up—and that methodology makes difficult—not explain them away at some altar of methodolatry.

Mental Models Bring Mental Imagery into the Mix

A second reason why the mental model notion is controversial is because it points to the phenomenon of mental imagery. More than that, it points to the complication that for some people some of the time, mental phenomena can be abstract whereas for some people some of the time mental phenomena can be imagistic (Doyle and Ford, 1998; Hoffman and Senter, 1978). Jay W. Forrester (1961) has referred to this mix of mental images with concepts and relationships. Psychology has had great struggles over this (see Hoffman, 1979). The unfortunate fact is that any theorizing that invokes a notion of mental imagery in order to explain things will upset some people some of the time, and if it acknowledges that mental imagery is itself a thing in need of understanding and explanation, these people will get doubly upset.

Mental Models are in a Fuzzy Relation to a Host of Other Mentalistic Concepts

A third reason why mental models are controversial is that they relate, in fuzzy ways, to a variety of terms used to capture various aspects of versions of mental models: scripts, schemata, cognitive maps, prototypes, frames, and so on. Mental models relate, in fuzzy ways, to a variety of terms used to denote hypothetical "types" of knowledge (declarative, tacit, procedural, implicit, verbalizable, non-verbalizable, and so on). Although history and philosophy of science might benefit from extended treatments, research might come to a quick halt were one to dwell on such matters and not get on with the work. Our goal in this chapter is immediate and practical. Above all, we seek to avoid ontological paralysis that, as we will show, can stem all too easily from theorizing about mental models.

Mental Models Fit the Macrocognition Paradigm, not the Microcognition Paradigm

A fourth reason why the mental model notion is controversial is that it does not fit comfortably with the agenda of mainstream cognitive psychology. In fact, the mental model notion can be used to highlight the distinction between microcognition and macrocognition. Many of the seminal theories in cognitive science are essentially linear causal chains. Examples are the Shannon-Weaver theory of communication (encoder→transmitter→receiver→decoder) which helped stimulate information-processing psychology (see Shanon, 1948), and Colin Cherry's (1955) filter model of attention. Though some early, and most later models had to have at least one loop (top-down as well as bottom-up processing), the basic search has been for causal sequences of mental operations that are believed to be somehow fundamental (for example, memory access, shifts of attention, and so on). The beginning assumption in macrocognition is that the primary functions and the supporting functions are parallel and highly interacting, with subtly different mixtures of primary and supporting functions involved for certain aspects or types of cognitive work. It is difficult to think of mental models in any "causal chain" sense, given their fluxing conjunctions of concepts, images, beliefs, and so on. And computational cognitive modeling seems to lie well beyond some far-off horizon. In fact, this aspect of mental models may be one reason why some in cognitive psychology might have trouble with the notion—they are trying to fit a macrocognitive phenomenon into the microcognitive mold.

Mental Models are Limited

A fifth point for contention has to do with the fact that mental models are said to be selective and incomplete (Forrester, 1971; Rouse and Morris, 1986). Scientists have asserted that mental models are limited to seven chunks (see Doyle and Ford, 1998, pp. 18–19).

We know that with sufficient practice at immediate recall for various kinds of materials (for example, strings of numbers, restaurant orders, and so on) the 7 ± 2 can be surpassed. How many state variables can a nuclear control-room operator consider at one time? After the human-factors practitioner has confidently offered 7 ± 2 as the answer, the engineers or designers ask, "What's a chunk?" The response is typically, "It depends on the domain and the person's experience." Studies of expertise have shown clearly that the amount of information people can integrate into chunks is both flexible and domain-dependent. This challenges the researcher to dig deeper to help the designer or engineer discover what might be the basis for "chunking" information in that particular domain. So although there might be constraints on the number of chunks people can deal with effectively in working memory, and although 7 ± 2 might be a good ballpark estimate of that constraint, 7 ± 2 is hardly a limitation in the sense of a practical bound on the span of immediate apprehension. Rather, it reflects how material comes to be meaningfully organized. The challenge for research is *precisely* to study cognition across the proficiency continuum to see how experts develop larger, organized, and functional chunks (Flach and Hoffman, 2003).

It would set an impossible benchmark indeed to say that only perfect mental models merit scientific scrutiny. At any one point in time, anyone's mental model, even that of an expert, is bound to have some simplifying and incorrect aspects. This has been called the "reductive tendency" (Feltovich, Hoffman, and Woods, 2004), a consequence of what it means to learn, rather than a bias or limitation.

Mental Models are Wrong

A sixth reason why the mental model notion is controversial has to do with the fact that mental models are often wrong. Specifically, in research on laypersons' and students' mental models, it is generally shown that mental models can be fuzzy, implicit, mostly wrong, vastly simplified, dynamically deficient, broad, amorphous, and so on. The stance of cognitive psychology, the psychology of reasoning, and related fields is that "the most noteworthy characteristics of mental models are the deficiencies that arise from bounded rationality" (Doyle and Ford, 1998, p. 11).

The "flawed" nature of mental models is of course the reason they came under scrutiny in cognitive psychology in the first place, especially for instructional design (see McCloskey, 1983). Johnson-Laird's mental model theory was in fact proposed in order to explain the errors people typically make when trying to answer even fairly simple logical syllogisms. In the human–machine interaction field, Norman (1983) has described mental models as "incomplete," "unstable," and "unscientific," and that people's ability to simulate is "severely limited." Doyle and Ford (1998) provide a litany of instances where people's mental models are incomplete and incorrect, on topics ranging from the causes of global warning, to the workings of computers, to people's understanding of risk. Indeed, mental models have even been defined simply as sets of misconceptions (Atman et al., 1994).

When transported into applications, this leads some to suggest that mental models should not be studied at all. In cognitive psychology, Kieras (1988; Kieras and Bovair, 1990) argues that since mental models are incomplete, they would not permit correct inference of the steps required to solve problems or operate devices, and so should not be used to form instructional methods and materials. In the field of human factors, Vicente (1999, Ch. 2) argues that ecological interface design should not be "driven by" an analysis of cognitive constraints, that is, interface design should not be based on compatibility with the user's mental model. The reason is that mental models, perhaps even those of experts, are limited, incorrect, and so on.

Is this characteristic of mental models a reason to not examine mental models at all? Or is it throwing out the baby with the bathwater? From the standpoint of Expertise Studies and Cognitive Systems Engineering, 'flawed' mental models are definitely worth studying to enhance our understanding of novice–expert differences, or knowledge across the proficiency scale. Furthermore, Keil (2003) has shown that people have a less complete understanding of the workings of the world around them than they realize, but that these compact accounts permit them to track causal structures and get the gist of key relations without getting swamped by details. Thus, mental models can be thought of as lean cognitive representations rather than cracks in the veneer of rationality.

Paralysis by Analysis

What all this comes down to is that mental models are hard to define; "available definitions are overly brief, general, and vague" (Doyle and Ford, 1998, p. 3). Indeed, mental models have been defined so as to embrace all of knowledge: "deeply ingrained assumptions, generalizations…that influence how we understand the world" (Senge, 1990, p. 8).

We do *not* define mental models so broadly as to be equivalent to world knowledge or "collections of beliefs" (Maharik and Fischhoff, 1993), but we believe that anything that a person knows may be foundational to a mental model, including some beliefs. (Philosophically speaking, "beliefs" are a mere fuzzy boundary away from "knowledge" anyway.) We do *not* ignore the root phenomenon by retreating to a behavioristic-ecological view that mental models are merely things that "mediate between perception and action" (Wild, 1996, p. 10). That does not explain, it explains away. We do *not* ignore the phenomenon by asserting simply that mental models are "mechanisms." That does not explain, it merely substitutes one reductionistic metaphor (machine) for another (model) which obscures the phenomenon.

Could our entire discussion of CTA methods proceed without any reference to the notion of "mental model?" Perhaps. We could substitute "organized knowledge structures" or some such term, or otherwise and try and reduce all the conceptual baggage (images, scripts, and so on) to a small set of terms people might agree upon and nail down. One might conclude therefore that the mental model notion offers no value added. But it would be a vain hope that everyone would agree, and that the concepts would help us out by gladly remaining immutable. Furthermore, the approach of semantic de-boning would not allow one to actually escape any of the philosophical conundrums. It would just shift the issues from one turf to another. What do you mean by "organized knowledge"? What is a "structure"? Our view is that the notion of mental model (the aspects or features that various scientists and scholars have noted) gets at the core of a salient and important mental phenomenon. Further, we believe that the "mental model" designation serves at least as well as other (equally problematic) designations cognitive scientists might prefer. In this light, this chapter can be understood simply as an articulation of some of the methods for going beyond observation of "pure behavior."

Moving from philosophical paralysis to methodological paralysis, mental models can be hard to study. Psychologists generally view the detailed study of mental models as a difficult and complex, if not impossible, task. According to this view, mental models are continually changing, and furthermore, efforts to elicit, measure, or describe them can themselves induce changes in mental models. When people are asked to report their mental models, they may fail to report them accurately for any of several reasons, for example, they simply may not be aware of all of the "contents" of their mental models; they may feel compelled to invent explanations and answers on the spot that did not exist until the question was asked; or they may deliberately or unconsciously change the answers to correspond to the answer they think the researcher wants to hear (Norman, 1983). (We can set aside for the moment the fact that *all* knowledge elicitation is a co-constructive process; see Ford and Bradshaw, 1993.) The methods that cognitive psychologists believe are necessary to address these problems and to minimize measurement error (replication, verification, validation, and so on) are labor-intensive, time-consuming, and expensive, and are therefore only rarely applied (Doyle and Ford, 1998, p. 10).

This can be quite paralyzing, with some scientists arguing that mental models should not be studied at all. For instance, Kieras (1988) argues that as people become more proficient, they rely less and less on mental models (the knowledge becomes tacit); hence, the construct of mental model is useless in explaining expert performance. All of our experience and research on domains of expertise cuts against this bleak claim. Our view is that expert performance cannot *possibly* be understood fully without studying practitioner knowledge and reasoning. The phenomenon of mental models is manifest in every domain we have studied, and in some domains experts can be quite articulate in talking about how they imagine events and project them into the future (Hoffman, Trafton, and Roebber, in press).

Furthermore, we have no qualms about presenting *new* methods that researchers might try out, methods that await further study and validation. If a psychological method had to undergo rigorous validation, verification, replication, and successful use *before* it was even discussed, science would never have gotten off the ground. Indeed, many of the classic paradigms of cognitive psychology only became classic *after* replication, cross-validation, and so on, when people tested and refined the new methods that had been presented in the seminal papers (for example, the Brown, Peterson, and Peterson paradigm, the Sternberg paradigm, and the priming paradigm).

Defining Mental Models

In the reach for clarity about mental models, a number of scientists have distinguished types of models (for example, Black, 1962; Norman, 1983; Hoffman and Nead, 1983) in order to clarify the relations of the model to the thing being modeled. There is the target system to be understood, the human's conceptual model of that target system, and the scientist's conceptualization of the human's mental model (a representation of a representation). We agree largely with Doyle and Ford (1988) who define mental models as mental representations, but this concept can be unpacked:

- Mental models are a phenomenon or "presentation" to consciousness, in other words, they are "accessible" or "declarative."
- Mental models emerge in the interplay of perception, comprehension and organized knowledge.
- Mental models are relatively enduring (that is, they are not strictly static, not strictly "structures," not strictly "stored" things).
- Mental models are not snapshots but are themselves dynamic.
- Mental models are representations of ("mappings") something in the world (often, some sort of dynamical system).
- The ways in which a mental model emerges is shaped by the regularities, laws, principles, dynamics that are known or believed to govern the "something in the world" that is being represented.
- We would add that mental models often have a strong imagery component, but this additional assertion links easily enough into the ones stated above.

This takes us to methodology by asserting further that:

- The mental representations can be inferred from empirical data. People may not be able to tell you "everything" about their mental models, and they may not be able to tell it well. But with adequate scaffolding in a cognitive task analysis procedure, people can tell you about their knowledge, or, the knowledge can be manifested in the cognitive work in which people engage.
- The mental representations can be analyzed in terms of concepts and their relations.
- The concepts and relations can express states of affairs and dynamics.
- The concepts and their relations can be depicted in some form (words, propositions, diagrams, and so on) that constitutes the scientist's "model" of the mental model.
- The depictions can be regarded as re-representations of organized knowledge.

This definition is not substantively incommensurate with other definitions, such as that of Johnson-Laird (1983), although we do not mix our phenomenology with information-processing metaphors (that is, we do not see much value in saying that mental models are representations of tokens of variables). Our definition is not substantively different from that offered by Greeno (1977), of mental models as relatively enduring representations that interact with new information in a constructive process of dynamic problem representation resulting in the phenomenon (comprehension or image) that is presented to consciousness.

What we focus on in this chapter is the *functionality* of mental models.

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The Functions of Mental Models

Mental models have the function of description and comprehension, but also projection to the future (Rouse and Morris, 1986, p. 351). The word "model" can be taken as a metaphor that is used to designate the core phenomenon precisely because the act of mental modeling is the apperception of a dynamic "runnable" event that can be mentally inspected, thought about, and projected into the future. Many scientists (for example, Rasmussen, 1979) have asserted that mental models are used to predict future events, find causes of observed events, determine appropriate actions to cause changes, and engage in "what-if" reasoning. For Rouse and Morris (1986), mental models are used to predict as well as describe and explain. Mental models help people to generate expectancies and to plan courses of action.

This is the meaning of mental model as a macrocognitive supporting function (Klein et al., 2003). Without accurate mental models, we would not be able to make sense of events. That is why it is so important to develop cognitive field research methods to study mental models, particularly the mental models of domain experts. Thus, to our definition above we add the following assertion: The depictions (re-representations of organized knowledge) can be studied through the applications of CTA.

Practical Methodological Challenges

Granted, the assumptions one makes about the features of a mental model can affect the methods used to study it, but if the history of experimental psychology (that is, concepts such as script, schema, and so on) shows anything, it shows that methods can be easily adapted to fit different theoretical predilections. No method used in experimental and cognitive psychology is adequate for eliciting, analyzing, and representing the full range of knowledge structures, schemata, scripts, and so forth that a person may bring to bear in describing, explaining, and predicting events. If we consider a mental model to include everything a person knows that could be relevant to describing, explaining, and predicting an event, the range of beliefs becomes unmanageable. Paralysis again.

But perhaps the vagueness is a sign that we are thinking about mental models at too abstract a level. Often, when a concept seems too fuzzy to be useful, that means it needs to be examined in specific cases. In this chapter, we will explore ways researchers might reveal and understand the mental models of the domain practitioners they are studying, and use depictions of the mental models to good result in applied research. Thus, when we refer to mental models, we are referring to both the human's conceptual model of that target system and the scientist's conceptualization of the human's mental model. These are coupled because the ways in which mental models are depicted (diagrams, propositions, and so on) are loosely coupled to the methods that are used to reveal them. For instance, the Concept Mapping knowledge elicitation method results in Concept Maps, sometimes referred to as "knowledge models" (Hoffman and Lintern, 2006). A card-sorting task in which people place related concepts into piles results in semantic similarity judgments that can be analyzed as semantic networks (for example, Schvaneveldt, 1990). A task in which people represent their understanding of some dynamic process results in process or event diagrams. The coupling is neither necessary nor strict, in that any of a variety of CTA methods can yield knowledge about domain concepts, events, principles, reasoning strategies, and so on (see Hoffman et al., 1995). The primary objective of this chapter is to offer some suggestions for how CTA methods can be used to describe mental models.

CTA for the Study of Mental Models

Arguably, modern CTA began in the 1970s with the introduction of Hierarchical Task Analysis (see Shepherd, 2001). The phrase "cognitive task analysis" emerged in a number of communities of practice in the United States in the late 1970s. Task Analysis was never void of cognition, with even the earliest forms of task analysis of the early 1900s having such descriptive categories as "decide" and "perceive." (A full history of task analysis is Hoffman and Militello, 2008.) CTA methods as we know them today (reviewed in Crandall, Klein, and Hoffman, 2006) include a number of types of structured interview, methods of work observation, and experiment-like methods, to elicit data that are analyzed and represented as a description of how people think about different types of activities. CTA methods have been used to describe decision strategies (for example, Klein, 1998) and knowledge (for example, Hoffman, Trafton, and Roebber, in press). We should be able to apply CTA to elicit and represent mental models for the design of interfaces, software tools, training programs, and so on. Not only do the technologies need to accord with how people think but they must also help people accelerate the achievement of proficiency: We need to be able to help people develop better, richer mental models.

In the following set of claims we suggest just one way to direct CTA research to more effectively capture mental models. Our presentation of methods is an invitation to explore. Many methods we discuss are tried-and-true, but some are new. In this chapter we do not consider issues of method reliability and validity.

Claims About CTA for Mental Modeling

Claim #1: CTA Studies can be Focused on Relationships

The search for mental models may become more tractable if we move down a level, and distinguish between different *relationships* that a person has a mental model about. Instead of thinking about global mental models, we can treat mental models as descriptions of relationships. This approach follows from the basic notion of propositional representations (attributable to Thomas Aquinas) as expressed on modern cognitive psychology by Kintsch (1974), Greeno and Simon (1988), and others, who posited that knowledge can be represented as concepts (objects, attributes, and so on) linked by relations to form propositions rather than associations. Moray (1987) proposed that learning a mental model of a system depends on learning the relationships among subsystems (also see Samurçay and Hoc, 1996; Staggers and Norcio, 1993).

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What sorts of relationships might we consider? Johnson-Laird (1983) contrasted the features of physical and conceptual mental models, and identified several types of relationships that needed to be captured in a representation: conceptual, temporal, spatial, and causal. Building on Johnson-Laird's framework, we can identify different types of relationships that could be captured by CTA: mental models about spatial relationships, conceptual relationships, causal relationships, organizational relationships, and so forth.

Claim #2: Each of These Kinds of Relationships is Captured Fairly Well by Different CTA Methods

Table 4.1 Matching CTA methods to conceptual relationships

- Conceptual domain—Concept Map
- Activity—cause-effect diagram or process diagram
- Spatial relationship—map
- Device activity—cause-effect sequences, diagrams, or stories
- Temporal relationship—script
- Organizational relationship—wiring diagram
- Dependency relationship—plan or storyboard

Table 4.1 illustrates how we might match CTA methods with types of relationships. Thus, a mental model of a domain might be captured by a Concept Map (Crandall et al., 2006; Novak, 1998). A mental model of a spatial relationship is best depicted by a topological diagram (for example, Gould and White, 1974). A mental model of a device—an image and related beliefs about the device's structure and functionality (the activity of the device)—might be described by diagrams accompanied by expressions of cause-effect sequences or even stories illustrating different device modes (for example, Norman, 1983). A temporal relationship might be illustrated by a script, an organizational relationship might be represented by a "wiring diagram" showing roles and functions, and a dependency relationship might be described by something like a plan.

Claim #3: We Can Select CTA Strategies by Clarifying the Purpose of the Research

Investigators rarely initiate a CTA study without some external motivation. The reason for the study can constrain and direct the research (Hoffman and Deffenbacher, 1993). One reason to conduct a CTA study is to identify critical cues that play a role in the formation and use of mental models. For instance, Crandall and Calderwood (1989) selected a CTA method to catalog critical cues in a study of neonatal intensive care unit nurses in spotting early signs of sepsis. Hoffman, Coffey, and Ford (2000) observed the weather briefings of forecasters to reveal the patterns in radar images that are indicative of tornado formation.

CTA methods can help the researcher to explain strategies. Thus, Klein, Calderwood, and Clinton-Cirocco (1986) explained how fireground commanders were able to make critical decisions under extreme time pressure and uncertainty.

CTA methods can account for errors by revealing flaws in mental models. As described earlier, McCloskey (1983) found evidence that people held an impetus theory that led them to expectations that violated principles of physics. Norman (1983) noticed that people were making unnecessary key presses on hand calculators (for example, clearing the registers by pressing the CLEAR key, not once, but several times). Norman found that people did not have accurate mental models of the devices; they acknowledged their inadequate mental models. Their inefficient behaviors were actually reasonable strategies that reduced memory burdens.

CTA methods can discribe the knowledge base of experts, which can be relevant for training and for system design. R. Hoffman, Trafton, and Roebber (in press) and Pliske et al. (1997) conducted CTA studies with weather forecasters for these reasons.

Claim #4: Researchers Draw on a Small Set of Paradigms

The challenge of designing CTA studies to capture mental models becomes more tractable when we identify the common paradigms used in these kinds of studies.

Mental models are foundational to a range of different types of cognitive performance, such as solving problems, generating predictions, anticipating events, and forming inferences. Investigators have developed many different types of experimental paradigms to measure success on these kinds of tasks. These are not the paradigms of interest here.

Claim 4 is about paradigms that uncover the content of a mental model, not just its impact on performance. Rouse and Morris (1986) listed several CTA approaches that could be useful in eliciting a person's mental models. These include the use of the Think-Aloud Problem Solving (TAPS) task, interviews, and questionnaires. Gentner and Stevens (1983) listed the methods they had encountered: TAPS, card-sorting tasks, cognitive psychology experiments, field observations, and so on. The taxonomy described below, which overlaps that of Rouse and Morris, as well as Gentner and Stevens, is based on a selective review of articles and chapters on mental model research:

- TAPS. Have participants think aloud as they try to solve problems.
- Directed inquiry. Ask participants to describe their reasons for adopting a regimen.
- Nearest Neighbor. Present several alternative mechanisms and have participants identify the one closest to their beliefs.
- Cognitive Intervention. Direct the participants to use a particular mental model (or metaphor) and see how that turns out.
- Glitch Detector. Uses bogus models of cognitive strategy to see if participants notice the error, and then compile improvements.
- Oddity. Look for unexpected activities during observations of cognitive work.

CTA Methods

By far, the most common CTA paradigm for revealing mental models is TAPS, usually combined with a data analysis method called protocol analysis. TAPS is not one single procedure but is a suite of methods, variations on a theme. The most widely cited method is the one preferred by Ericsson and Simon (1993). It involves TAPS, but has come to be equated with "protocol analysis" since that is its given name. In their procedure, the experimenter presents the participant with a problem and asks the participant to think aloud. In other variations on TAPS procedures, participants can be asked questions while they work. They can be interviewed afterwards, and so on. (For examples of such variations on TAPS methods, see Beach, 1992.)

Ericssonian protocol analysis is aimed at getting a running record of the "contents of working memory" from which the researcher derives a description of problem states and operators. But it can also be used to describe knowledge and reasoning, that is, mental models. The mental models serve to explain why participants did what they did in the course of problem solving (see Beach, 1992).

Another form of Think-Aloud Problem Solving can provide insights about the processes people use in building and revising mental models, as well as about the content of the mental model. Thus, for example, Collins and Gentner (1987) asked people questions about the physical process of evaporation, and found a frequent use of mini-analogies as the participants used mental simulations to construct story accounts. They described their participants as using visual images and having an "introspective feel of manipulating images" (p. 246).

Researchers commonly set up comparisons. They may recruit different types of participants, such as experts and novices, in order to contrast the sophistication of their mental models. For instance, a task in which people sort domain concepts according to their perceived semantic relatedness can reveal the deep versus superficial understanding of problems (for example, Chi, Feltovich, and Glaser, 1981). Researchers may contrast mental models formed for easy versus difficult problems. They may contrast participants from different cultures. They may contrast performance before and after instruction that is designed to alter mental models. Another type of comparison observes the nature of the mental models from the beginning of a training program to the end.

A great deal of preparation often goes into the design of the problems that are presented to participants. Problems must be carefully selected and designed in order to address the research purposes (for example, the mental models of pilots faced with a simulated malfunction; Smith, McCoy, and Layton, 1993). Furthermore, the process of transcribing the verbalization and encoding the transcript statements is effortful and time-consuming (Hoffman et al., 1995).

In addition, the researchers usually need to be familiar with the domain and the problem set, and should try to anticipate the mental models that participants might be using. This is particularly true in designs that permit the data collectors to question the participants as they go about solving the problems. The kinds of tip-offs they look for, the kinds of hypothetical questions they pose, the ways of phrasing an inquiry without leading the participant, all require skill, knowledge, and also sophistication. Even designs that postpone the questioning until after the participant completes the problem place demands on the data collectors to avoid leading questions or hints. This is a reason why some researchers shy away from the use of probe questions and advise others to shun such methods (for example, Ericsson, 2006). As we know from studies of expertise, expertise is achieved only after practice, and more practice. The same holds for those who would try out alternative methods of CTA, including interviews and TAPS tasks that rely on probe questioning. As we have pointed out, our choice is to explore rather than let ourselves be paralyzed because a method cannot be formed as a controlled experiment.

Directed Inquiry methods (see Table 4.1) use interviews to see if participants can explain their reasons for adopting a strategy. This method can rely on specific procedures such as the Critical Decision method (Klein et al., 1989). For example, Lippa, Klein, and Shalin (2008) have conducted interviews with patients diagnosed with Type II diabetes to learn how they control the disease. The interviews try to uncover the patients' belief systems about the causal relationships between the disease, the various interventions for controlling it, the dynamic life events that complicate any control regimen, and the effect these strategies and adaptations have on the patients' blood glucose levels. The interviews may elicit general beliefs, or they may probe beliefs in the context of specific incidents, such as recent self-management activities, adaptations to stress, illness, and other complications that the patients report.

The Nearest Neighbor technique is particularly helpful in cases where people cannot readily articulate their beliefs. For example, Klein and Militello (2001) describe using this method with housewives to examine their beliefs about how a common item works. The housewives gave blank stares when asked about how the item did its job. Subsequently, the researchers prepared a set of alternative diagrams, each depicting a different possible mechanism. The next group of housewives was asked which diagram came closest to matching their understanding for the product. This time, the housewives had little trouble selecting the best match, and then explaining where it didn't quite fit. Hardiman, Dufresne, and Mestre (1989) used a similar method with physics experts and novices. They presented the subjects with a target problem and asked them to judge which of two comparisons would be solved most similarly.

The Cognitive Intervention technique is often applied as a form of instruction. For example, Gentner and Gentner (1983) suggested analogs to people who were trying to make sense of electricity. The nature of the analog that people used (flowing water or teeming crowds) influenced the type of inferences they were able to accurately make.

The Glitch Detector method presents participants with a diagrammatic account of a process, but with a twist—the diagram has a subtle but important flaw. The participant is not told that the diagram is flawed. The task is simply to review the diagram. The question is whether the participant notices the flaw—to indicate the way the participant understands the process.

A variant of the Glitch Detector method is the Macrocognitive Modeling Procedure (perhaps more appropriately thought of as a cognitive mis-modeling procedure) (Hoffman, Coffey, and Carnot, 2000). The Macrocognitive Modeling Procedure was designed in an attempt to create a "fast track into the black box," in the sense of supporting the development and behavioral validation of macrocognitive models of

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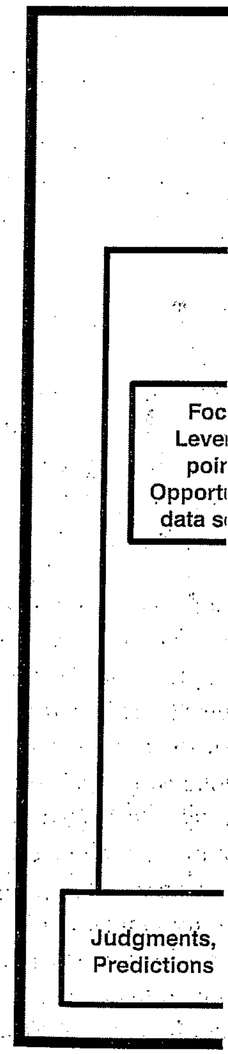

Figure 4.1 **Th**

Source: Adapted frc

practitioner reasoning in an efficient way, avoiding labor-intensive protocol analysis. The MMP evolved after Hoffman, Coffey, and Ford (2000) had created the "Base Model of Expertise," presented in Figure 4.1. This model combined the notion of the hypothesis testing refinement cycle, from Karl Duncker (Duncker, 1945; see also Newell, 1985), combined with other key macrocognitive functions that can be seen in Figure 4.1. The model seems to capture (as variations on a theme) a number of proposed hypothetical reasoning models that have been offered in studies of diverse domains of expertise. (See Hoffman and Militello, 2008, Ch. 8.)

The MMP was first conceived when a weather forecaster was shown the Base Model of Expertise, and was asked whether it seemed appropriate to the domain. It was felt that this would be sensible since, as we pointed out earlier, the weather forecasting community is comfortable with the distinction between

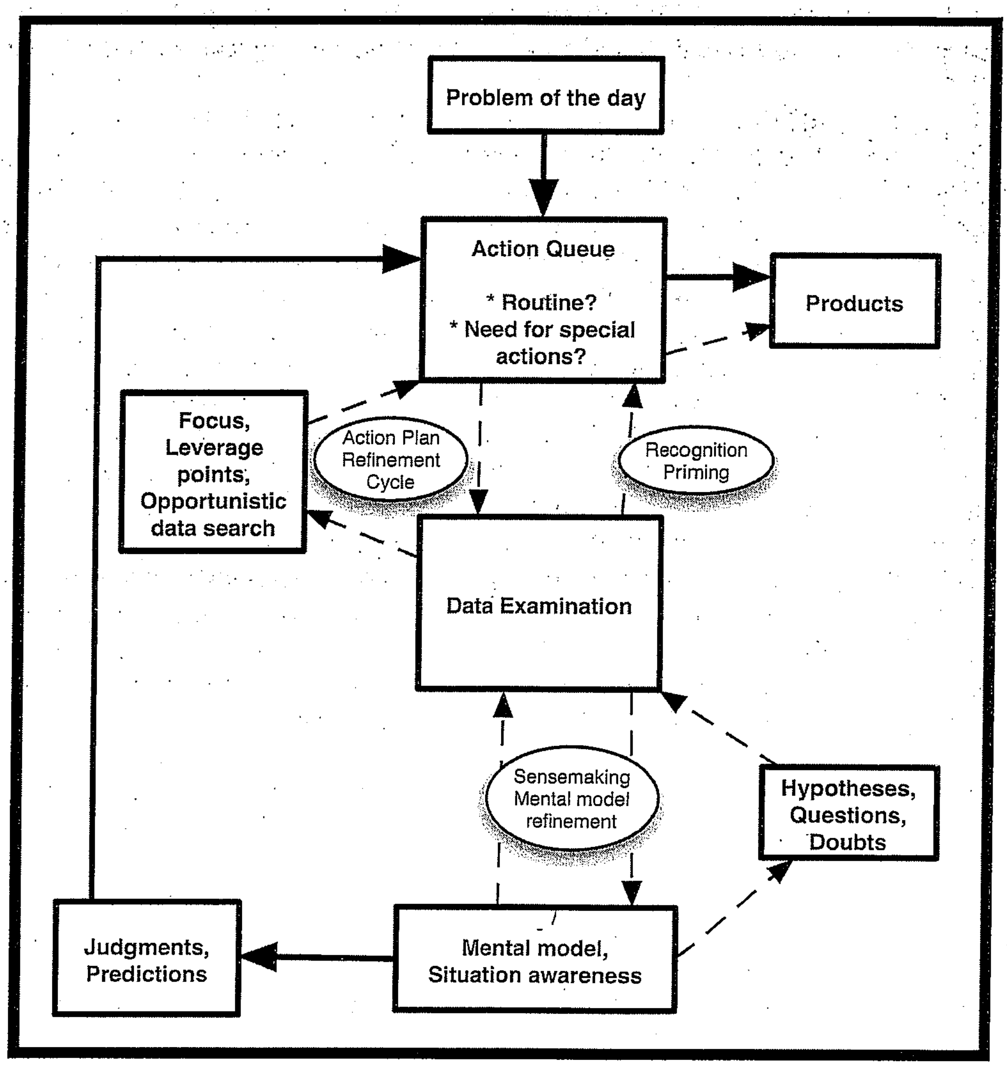

Figure 4.1 The "Base Macrocognitive Model" of expertise

Source: Adapted from Trafton and Hoffman, 2006.

conceptual models versus computational models. The informant spontaneously took the diagram as an opportunity to add domain-specific details to the process description, and modify some of the relations among the diagram elements. With this experience as the starting point, Hoffman et al. created a formal procedure, consisting of four steps:

- *Step 1*: The researcher takes the Base Model and adjusts it to make it directly pertinent to the domain. For example, the "Problem of the Day" would be specified as "The Forecasting Problem of the Day" and "Data Examination" would be specified as "Examination of images, data, radar." Next, two alternative "bogus models" are created. At least one of these includes some sort of loop, and both include a number of the elements of the Base Model. Taken together, the bogus models include all the core macrocognitive functions (for example, recognition priming, hypothesis testing, and so on). Ideally, bogus models are not too unrealistic, yet the researcher would expect the practitioner to not be entirely satisfied with either of them.
- *Step 2*: Domain practitioners, spanning a range of proficiency, are shown the bogus models and are invited to pick the one that best represents their strategy. Then, using the bogus models and their elements as a scaffold, the practitioners are invited to concoct their own reasoning diagram.
- *Step 3*: After the individual diagrams have been created, the researcher deliberately waits some weeks, or even months, and then each practitioner within the organization is shown all of the models and is asked to play a "guess who" game. This step in the MMP is a form of sociogram, revealing the extent to which practitioners share their knowledge and discuss their reasoning. This step also helps to identify individuals who possess special sub-domain expertise or skills. Thus, this step contributes to proficiency scaling.
- *Step 4*: After another delay, the researcher locates him or herself in the workplace and observes each practitioner as they arrive and begin their day's work. Some elements of the model can be validated by observing worker activities (for example, "examine satellite images"), whereas other elements that cannot be validated that way can be the focus of probe questions. Figure 4.2 shows results for just one of the practitioner models, indicating the results in Step 4. The call-out balloons indicate the results from the observation/probe questioning procedure.

Results from the procedure included models of the reasoning of journeymen and expert forecasters, affirmed based on observations of forecasting activities. It took an average of 52 minutes task time to develop a model. This is without doubt considerably less than the time taken in other methods. (For example, preparing and functionally coding a transcript of problem-solving protocol can itself take many hours.) The results conformed to the Base Model of forecaster reasoning that had been developed in an initial documentation analysis. The results also clearly revealed differences in proficiency, with less experienced forecasters relying in an uncritical way on computer forecasts, and less likely to think hypothetically and counterfactually.

This study was a first attempt, and limited in sample size, but promising enough for us to invite others to try it out. (Details on procedure, instructions, and so on can be provided by the second author upon request.) We feel that the MMP holds promise for the development of reasoning models and the testing of hypotheses concerning reasoning models in less time than taken by traditional experimentation. It also showed that it makes little sense to think that a single macrocognitive model will effectively capture practitioner reasoning. One forecaster, in Step 3, rejected his own

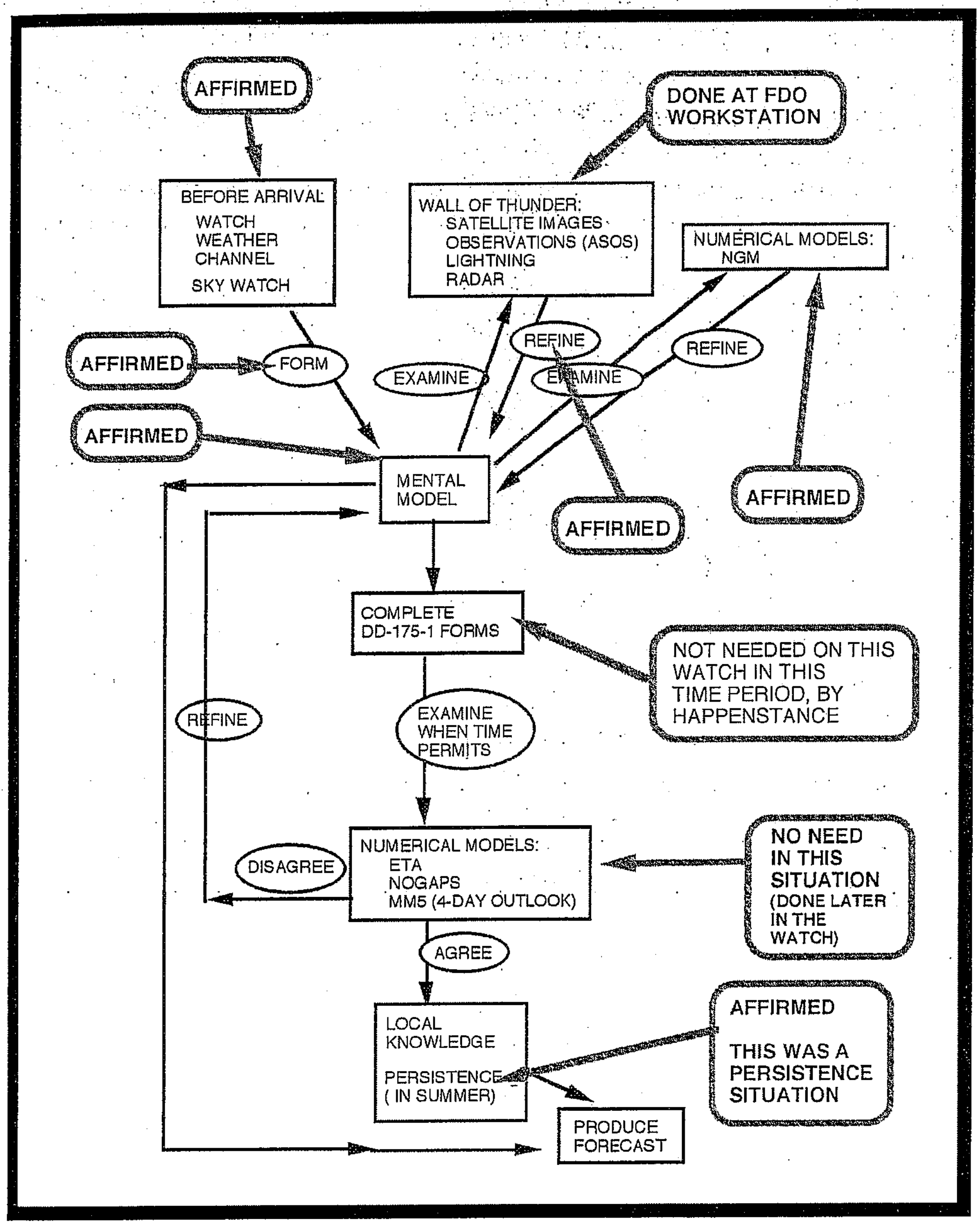

Figure 4.2 **One of the forecaster models, with annotations from Step 4 of the MMP**

model, later explaining that the model that had been created weeks earlier was no longer his preferred strategy since the weather regime had changed. Hoffman et al. speculated that for a domain such as forecasting, many dozens of "strategy models" would be needed to present a rich and fair picture of practitioner reasoning.

Additional observational methods of work analysis are described in Crandall, Klein, and Hoffman (2006), and in Hoffman and Militello (2008). For example, the Oddity strategy is employed during field observation. In this method, the researchers remain on the lookout for activities that don't seem to make sense. These are windows into alternative mental models. Thus, Norman (1983), in observing people using hand calculators, observed seemingly inefficient and unnecessary behaviors that revealed gaps in the mental models of how the devices worked. Hutchins (1983) described the way that he was able to understand Micronesian navigation by being sensitive to practices that made little sense from the standpoint of Western navigation. Hutchins assembled the evidence, in the form of many individual, related observations, and synthesized these to formulate his hypothesis about the mental models of the Micronesians. Darnton (1984) described the value of an Oddity strategy for historians, who can never really understand the way people from earlier eras reasoned. Darnton deliberately used unusual and baffling events as his point of departure. He was able to break out from his own mental models to gain a deeper understanding of the mental models used during a given historical period.

Conclusions

The concept of mental models becomes somewhat manageable if we focus our inquiry on knowledge people have about specific types of relationships (for example, conceptual, spatial, temporal, organizational). CTA methods already exist, and have been applied in multiple contexts or domains for eliciting and representing knowledge of a variety of relationships. Moreover, researchers have developed a set of paradigms for studying mental models. We can identify a number of next steps that might improve our research into mental models.

It could be valuable to compare different CTA methods that are relevant to a type of conceptual relationship, and see their relative strengths and weaknesses regarding yield of information, validity, reliability, effort, efficiency, and so on (see Hoffman, 1987). Such work could improve the effectiveness of CTA methods. It might also suggest new forms of CTA that might describe important relationships that are not handled well by our existing methods.

Another direction is to survey the techniques used by researchers to code data. Ford and Kraiger (1995) identified several ways of scoring data. One method was to score levels of complexity, a measure that might reflect the level of sophistication of a mental model. Additional measures were the degree of differentiation among elements within a level, and the distance or number of links between key nodes. Recent developments in Concept Mapping (for example, Cañas, Novak, and González, 2004) expand the range of potential measures reflecting the quality and complexity of mental models. As with any mapping of a conceptual definition to a measurable, caution is in order because the quantitative analysis is only a snapshot of a qualitative understanding (in this case, of the content of a mental model). Dekker (2003) has warned about categorization that appears to provide insight but is insensitive to the dynamics of each individual case. For that reason, we need to be careful to use the qualitative findings of CTA studies along with quantitative measures.

Finally, researchers can focus their work on specific criteria for effective mental models. For example, Lippa and H.A. Klein (in preparation) are finding variations in comprehensiveness (whether the mental model includes all the relevant processes involved in the causal relationships), depth (the level of detail of how each major factor operates), coherence (whether the mental model forms a compelling metaphor or story, as opposed to a set of fragmentary beliefs that are not well-integrated), accuracy (whether the mental model contains flawed beliefs), and utility (whether the mental model is helpful in maintaining a regimen for controlling the diabetes). Other researchers might use different criteria. We expect that the using of criteria can guide the examination of mental models, and that the nature of the findings will shape the criteria as the study progresses.

In a few places in this chapter we have referred to issues of validation and verification. Our stance is to avoid paralysis resulting from the refusal to try a method unless it has been proven to reliable and at least up to the methodolatrist standard of 'observation of pure behavior' (whatever that might be). All methods, even experimental procedures, have strengths and weaknesses, and the potential weaknesses should not prevent people from exploring the methods as opportunities. This being said, of course validation and verification are important. There are other issues as well, such as ethical ones. For instance, in some contexts, Step 3 of the MMP (the "guess who" game) may raise issues of disclosure.

We look forward to an expansion of research on mental models that can elaborate and extend the methods we have described, provide guidelines for using CTA methods to capture mental models, and also increase our understanding of macrocognitive functions.

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We would like to thank Beng-Chong Lim for posing the initial questions about using CTA to study mental models—questions that started us down this path of exploration. The second author's contribution was through participation in the Advanced Decision Architectures Collaborative Technology Alliance, sponsored by the US Army Research Laboratory under Cooperative Agreement DAAD19-01-2-0009. Thanks to Jan Maarten Schraagen and Tom Ormerod for offering insightful and challenging comments on a draft of this chapter.

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